

instance of the lower portion of the erection being inclosed by matting to form a "ground floor." Were these pile-dwellings confined to the low, flat lands upon which the Bengali delights to place his paddy-fields, it would be obvious that they were adopted for the purpose of obtaining a dry, wholesome floor, and security against unanticipated floods. But so far is this from being the case that only very rarely is a Naga or Kuki village to be found on low-lying ground, and generally they are to be seen upon the sides and even the summits of considerable elevations, where any danger from floods is quite out of the question. Again, it might be supposed that these elevated dwellings were adopted as a protection against wild animals but for a curious practice occasionally observable amongst the hill-men. This is the habit of building upon the steep side of a hill in such a manner that the back of the dwelling rests directly upon the ground, while the front is supported upon piles which are of a height sufficient to render the floor, throughout its length, horizontal. Such a plan as this reduces the protection afforded from vermin and wild animals to a minimum, and seems to justify the belief that the fear of these creatures at least could have little or no influence upon the architectural habits of the hill-tribes of this part of India; and I long ago came to the conclusion that here at least the object of the pile-dwellings was simply to attain in the easiest way a floor which should be exempt from the damp exhalations of a tropical soil.

JAMES DALLAS

"Probable Nature of the Internal Symmetry of Crystals"

UNDER this head Mr. Barlow has published in NATURE of December 20 and 27, 1883 (pp. 186 and 205) an interesting and ingenious memoir. The subject being very important, but also very difficult and intricate, a discussion of the new theory may perhaps contribute to render our ideas a little more precise.

Whilst Häuy, Frankenheim, Delafosse, Bravais, and others think a crystal built up of mere congruent particles, which may be either the chemical molecules or rather certain aggregates of them, Mr. Barlow considers the arrangement of the different chemical atoms in the interior of a crystallised compound, and illustrates some facts by this manner of viewing them. I purpose in the following submitting some objections which arise against the deductions of the author. These objections are of a geometrical, chemical, and physical nature; let us begin with the geometrical ones.

The first problem of Mr. Barlow is "to inquire what very symmetrical arrangements of points or particles in space are possible." He comes to this result: "It would appear that there are but five." Then he describes these five arrangements. What conditions are to be fulfilled by an arrangement of points in space which is to be "very symmetrical," is nowhere said. According to this indefiniteness of the fundamental notion, the five kinds of very symmetrical arrangement seem to be found rather by divination than by systematic reasoning. Therefore the foundation of the theory appears somewhat arbitrary; and we may suspect that it is incomplete. We are in fact confirmed in this presumption if we consider the results of a geometric research published in my "Entwicklung einer Theorie der Krystallstruktur" (Leipzig: Teubner, 1879). In this book I have specified all possible arrangements of points that are regular and infinite, I have called a system of points *regular* if the points are disposed around every one point of the system in precisely the same manner as around every other. *There are sixty-six such regular systems of points possible.* According to the peculiarity of their symmetry they are subdivided into groups, which correspond strictly to the known crystallographic systems. Many of those arrangements of points have a hemihedric or tetartohedric character; others have the structure of a screw; and amongst the latter I could even suggest one particular system which represents the internal structure of quartz. The latter result was obtained (*loc. cit.* pp. 238-245) by comparing the crystallographical and optical properties of quartz with those of the known combination of thin laminae of mica arranged in the manner of winding-stairs, described by Prof. Reusch fourteen years ago. All sixty-six systems are in agreement with the principal law of crystallography, the law of rational segments of the axes (Wiedemann, *Annalen der Physik*, 1882, vol. xvi. p. 489). For example, if we have reason to suppose that a certain one of these systems should represent the structure of a given substance crystallising in hexagonal pyramids, then we derive geometrically the same series of possible pyramids which nature actually exhibits.

Four of Mr. Barlow's five kinds of "very symmetrical arrangements" prove to be extremely particular cases of four general systems of mine. The first, second, and third kinds of Mr. Barlow's result from the systems which I have called the "rhombendodecahedric, cubic, and octahedric system with 24-points-aggregates" ("Entwicklung," pp. 165-168), if we suppose the twenty-four points of the so-called "24-punkter" coinciding in one point, and if we identify this point with the centre of a sphere of Mr. Barlow. Mr. Barlow's fourth kind of "very symmetrical arrangements" results as a particular case from my "3-gängiges 6-punkt-schraubensystem" (*loc. cit.*, Fig. 46), if the sides of all hexagons are supposed to touch one another, and the layers to have convenient distances. Mr. Barlow's fifth kind of symmetry, not being regular in the sense defined above, cannot be found amongst my sixty-six systems. Though every point is surrounded by six neighbouring points at equal distances, the latter have not throughout an identical arrangement. Every point of the first, third, fifth, &c., layers is situated at the centre of a perpendicular prism (with regular triangular base) whose angles bear the six neighbouring points of the system, but around every point of the second, fourth, sixth, &c., layers, the six neighbouring points are situated at the angles of two regular triangles, which do not lie parallel over one another as before, one of them being turned round in its plane 60°.

As my sixty-six systems comprise four of Mr. Barlow's kinds of symmetry, it may be expected that they include other arrangements besides, which may also pass as "very symmetrical." For example, in a cubic aggregate of points, the centres of the edges of all cubes determine a very symmetrical arrangement of points, where every point has equal distances from the next eight surrounding points (cf. "Entwicklung," &c., p. 160). From this I believe I have shown that the geometrical foundation of Mr. Barlow's theory is somewhat arbitrary and incomplete.

I now come to the chemical objections, which I will explain by an example. A chemical compound of two kinds of atoms, present in equal number—for example NaCl—could, according to Mr. Barlow, crystallise into the first or second of his five kinds of symmetry, for either of these two kinds allows the regular arrangement of two kinds of particles in equal number. In the first kind of symmetry (for example) spheres are so arranged that they constitute a cubic system of points, in which the centre of each cube bears also a point of the system. By putting atoms of one kind (Na) on the angles, and atoms of the other kind (Cl) on the centres of the cubes, we have built up the structure of a crystal of NaCl. Thus eight atoms of Na stand in exactly identical manner around an atom of Cl (and also eight atoms of Cl around an atom of Na). The atom of Cl seems consequently to be in equally close connection with eight atoms of Na; it has exactly the same relation to these eight atoms. It appears therefore as *octovalent*, certainly not as univalent; for it would be entirely arbitrary to suppose any two neighbouring atoms of NaCl in an especially close connection and to take this couple for the chemical molecule of NaCl. By this example we see that from Mr. Barlow's point of view both the notion of chemical valency and of chemical molecule completely lose their present import for the crystallised state. This objection, of course, will not destroy the theory of Mr. Barlow, since chemical valency does not yet belong to perfectly clear and fixed notions, and since the idea of the chemical molecule in a crystal is also not evident and clear. The author, however, is at all events obliged to show why these two notions, of such great moment for substances in a gaseous state, should become completely insignificant, as soon as crystallised bodies are in question.

Finally for a physical objection. With respect to the fact that most substances change their volume in congealing, Mr. Barlow admits that the atoms themselves undergo an expansion (positive or negative) in the act of crystallisation. Thus he attributes to the atoms variability of volume, *i.e.* one of those qualities, for the explanation of which the atomic theory has been devised. Well, let it be so, but this hypothesis of atomic expansion is not even found sufficient everywhere, but must be assisted occasionally by auxiliary hypotheses. Thus for explaining the isomorphism of substances which contain atoms of the same kind (*e.g.* CaCO_3 and FeCO_3) Mr. Barlow supposes that the expansion in the act of crystallising is confined to the common atoms, whilst the different atoms in both substances remain unaltered.

All these objections do not overthrow the author's theory, but they shake it. Perhaps they will induce Mr. Barlow to establish

his theory in a more solid and more general way, and in this case also I shall have attained my aim.
L. SOHNCKE
University of Jena

Holothurians

THE observations which I made in 1883 among the coral-reefs of the Solomon Group on the habits of the Holothurians support the view that these animals do not subsist on living coral. I carefully examined the material voided by about twenty individuals, and found its composition to be of a mixed character. In addition to the calcareous sand and gravel which formed its bulk, there were numerous tests of the large foraminifer—Orbitolites—and several small univalve and bivalve shells, besides the joints of a stony alga and the operculum of a young nerite, &c. This observation is supplementary to those contained in my previous letter on this subject (NATURE, vol. xxvii. p. 7).

Traders in this group tell me that when collecting a species known in the trade as the "large tit-fish," they have frequently found a small eel inside the animal, which usually escaped before it could be secured. One man received a smart electric shock, whilst handling a trepang containing one of these eels.

H. B. GUPPY

H.M.S. *Lark*, Auckland, N.Z., January 1

Unconscious Bias in Walking

SURELY Mr. W. G. Simpson has written from imperfect memory when he tells us in NATURE (vol. xxix. p. 356), "if the majority of people, as Mr. Darwin argues, are left-legged, they would circle to the left in a mist, as Mr. Larden says they do." In Mr. Larden's letter (p. 262) the following passage occurs: "This theory (his own) involving as further consequences that those in whom the left leg is the strongest would circle to the right," &c.; again, "I myself am right-legged and in a mist I always circle to the left." Although Mr. Simpson has misquoted Mr. Larden, he has arrived at the same conclusion that I did (see NATURE for January 31, p. 311), but gives his views in different words, namely, that "there is a bias towards the stronger limb, irrespective of length."

JOHN RAE

The Storm of January 26

THE lowest reading, reduced to the sea-level, of the barometer here, about six miles south-east of Omagh, during the gale on Saturday, the 26th ult., was 27.68, and occurred at 4.15 p.m. Dublin time.

ROBERT DIXON

Clogherny, Beragh

PALESTINE EXPLORATION

THE following communication has been forwarded to us for publication:—

Mediterranean Hotel, Jerusalem, January 18, 1884

DEAR PROFESSOR OLIVER,—A chest in a waterproof cover leaves here to-morrow for London to Messrs. Cook and Son, Ludgate Circus. It should arrive on February 25 or sooner, and I have directed that it should be forwarded immediately to Kew. I hope to arrive soon after. It contains all my dried plants. They are made up in various packages, with localities written outside. Of course you will have them kept dry and looked after, but I think they had better not be overhauled until I come, as I should like to open them as they are, while the contents of each package and its associations are fresh in my memory. The earlier desert plants are in many cases only valuable for recognition, I fear, as they are withered remains, but I frequently obtained a lingering flower and many seeds. All my seeds and bulbs I have sent according to promise to Mr. Burbidge, of the College Botanic Garden, Dublin. In the mountains about Sinai and Jebel Catherine I obtained better specimens, and things gradually improved to Akaba. We got through a good deal of unexplored country and had a most efficient conductor. Along the Wady Arabah I made frequent detours into the mountains on either side, and was espe-

cially fortunate in having a good collection on Mount Hor and at Petra and its neighbourhood. The flora of Mount Hor (5000 feet) is extremely rich—a warm sandstone. I also collected mosses and lichens in the desert, and am still gathering all I can. My collections reach to here, including a run down to the Jordan. The pace is now (horses) often too rapid, but the camel was an admirable companion on a long march. We were delayed in the Ghor-en-Safiet, at the south-east end of the Dead Sea for ten days, an unparalleled sojourn in this most interesting place. It was early a little, but I made large collections there, and was very glad of the difficulties that opposed our departure. I found many unexpected plants—three ferns, for instance, on Mount Hor, and a *Stapelia*. I knew the names of very few of the things, and had no books, but Redhead and Lowne's papers were a help, though they gave a very poor idea of the real state of affairs. There is a fine *Acacia* in the Ghor-en-Safiet, distinct in many respects and far finer than *A. seyal*. It is the true "scent" about which there seems a lot of confusion. Hoping my collections will be satisfactory,

I remain yours very truly,

(Signed) HENRY CHICHESTER HART

P.S.—Here in Jerusalem there are about six plants in flower; down below in the Jordan I gathered about a hundred two days ago!

(Signed) H.

FAIRY RINGS

THE dark green circles of grass known as "fairy rings" formed the subject of a paper in the *Philosophical Transactions* of the new-born Royal Society in 1675; but it was only last year that the Rothamsted chemists, Messrs. Lawes, Gilbert, and Warington, announced what is no doubt a correct explanation of these phenomena.

The original theory of the electrical origin of the rings was succeeded by that of "chemical causes" propounded by Dr. Wollaston at a meeting of the Royal Society in 1807, and by Prof. Way in a paper read to the British Association in 1846. Besides the "mineral theory" which was here pressed into the service of a discussion that commenced, as already stated, more than two hundred years ago, De Candolle applied his famous "excretory theory" to its elucidation. At Rothamsted, however, the causes of fairy rings were still regarded as having been unsatisfactorily explained.

Sir John B. Lawes and his colleague Dr. Gilbert commenced their inquiries on this subject many years ago. Almost from the commencement of their experiments at Rothamsted they had regarded the alternate growth of fungi and grass as a striking example of what may be called the "natural rotation" of crops. As long ago as 1851 they described fairy rings in the *Journal of the Royal Agricultural Society* as "a beautiful illustration of the dependence for luxuriant growth of one plant upon another of different habits." It will be remembered that the experiments at Rothamsted led to the substitution of what is called the "nitrogen theory" for the "mineral theory" of former days, and practical agriculturists who know the value and the cost of nitrogen as an all-important agent of fertility will learn, perhaps without surprise, that the rich verdure of a fairy ring is due to the effect of nitrogen. Nitrogen is the *sine qua non* of plant growth, and fungi require a large amount of it. From what source do they obtain it? At the present time few, if any, chemists would maintain that they obtained it by the absorption of free nitrogen from the atmosphere, but in 1851 the eminent investigators at Rothamsted attributed the nitrogen of the fungi to their extraordinary power of accumulating that substance from the atmosphere; and this they thought enabled them to take up the minerals which the grasses, owing to